

1886)—*Yesso*—Hakodate, VIII. 1886 (*Leech*). *HONDO*—Yokohama¹. *KIUSIU* (*Leech*, 1890)—Satsuma, V. 1836 (*Leech*).

This is not a true *Argyroploce*, as it does not possess a thoracic tuft; it cannot be referred to *Enarmonia*, as at present constituted, since veins 3 and 4 of the hind wings are connate, not stalked.

[To be continued.]

XXXIII.—*Note on Variation in the Weasel and Hedgehog.*

By G. E. H. BARRETT-HAMILTON.

I HAVE read with much interest the two notes by Dr. Einar Lönnberg which appeared in the 'Annals' for May and June 1900. In these days of much writing and little attention it is pleasant to find that one's work attracts notice, even if that notice be critical or condemnatory. It is doubly valuable to receive criticism from one who resides in Scandinavia, a country whose climate and configuration lends itself in a very peculiar manner to the formation of local subspecies of mammals, or, in other words, to variation.

Before noticing Dr. Lönnberg's remarks I may say, by way of preface, that in my studies of European mammals my main object has been, first to record, and secondly to throw light, however dimly, upon the origin of the numerous variations which occur. The making of species or subspecies is therefore to me of quite subsidiary importance, and I care not a jot whether the forms upon which I find it necessary to bestow technical names be styled species, subspecies, races, forms, or phases. On the whole I incline to the latter word; but the use of the term subspecies is now so general and, I had thought, so well understood that I have found it convenient. No one who works for any little time at such matters can fail to meet with numberless difficulties or to notice the inequality between the various subspecies. That, however, is the fault of the system, or, if you like, of the animals themselves, which refuse to accommodate themselves to any scheme which man can invent, and which consequently excite the frequent protests of those who fail to see the troubles which must beset any system of minute inquiry. Even, however, if I were to find that I had made numerous

bad subspecies, I would vastly prefer to be on the side of those who attempt to unravel the mysteries of variation (it may be a task heavier than the cleaning of the Augean stables) rather than to cultivate the icy scepticism of the modern school of "lumpers," to whom the many phases of animal variation are like the ripples of the ocean to the sailor—things to be detested in proportion as their magnitude makes them troublesome. By such a school no real progress can now or ever be made.

Let us look at Dr. Lönnberg's criticisms. Admitting that the meaning of a subspecies varies somewhat with different authors, he supposes "that even in a subspecies the distinguishing characteristics (although they are of less importance than specific ones) must be constant to a certain degree and inherited from one generation to another; in the opposite case it is only an individual variety. Such independent individual varieties must not be called subspecies, in my opinion, even if they are numerous and dominate in some region."

Reading my introductory remarks, it is easy to understand where Dr. Lönnberg and I differ and where we agree. Finding that the weasels of the far north turn white in winter, while those of the south do not, I apply to each of these, in their extremes highly distinguishable, phases of the same animal a different trinomial name. This I find to be the best method at my disposal of calling attention to such differences. Dr. Lönnberg, on the contrary, prefers to minimize the importance of these really important colour changes by refusing to accord them the hall-mark of nomenclatural distinction.

But it is not this which puzzles and annoys Dr. Lönnberg so much as the existence in Scandinavia of weasels belonging to both of these forms. Well, why not take things as they are, and admit the difficulty, with the impossibility of ever completely surmounting it? Is it altogether preposterous that, while we have the regularly white-turning *Putorius nivalis typicus* in North and Middle Sweden, and the always brown *P. n. vulgaris* in Scania (connected, as we know they are, by various intermediates), we should similarly find the *P. n. typicus* on a mountain-top and the *P. n. vulgaris* in the valley of the same parish? Is not all this due to the same laws of climatic variation, and need it deter us from further investigation to find that such variation is in the highest degree perplexing? Shall we not rather do well to

accept the situation, pregnant as it is with interest, and to welcome each step in the road to its explanation?

It is, in fact, only to be expected that a mountainous country like Scandinavia, with one flank fully exposed to the damp blasts of the Atlantic, the other chilled by the near proximity of a vast continent, should present us with at least two or even more phases of each common mammal. This is certainly the case, as Dr. Lönnberg points out, with the polar hare, *Lepus canescens*, and I have shown that it is so also with the squirrel and also probably with the long-tailed field-mouse, *Mus sylvaticus*. Why, then, should Dr. Lönnberg be at such pains to demonstrate the occurrence of the "vulgaris" form together with the "typicus," an incident which was not only probable but necessary for the proper appreciation of their rôle as subspecies? And why should Dr. Lönnberg regard such intergradation both of colour and size as rendering the distinguishing characteristics derived from them unimportant, when in the very same paragraph he shows his thorough agreement with me that "the variation of the weasel certainly does not lack significance, because intermediate stages occur which unite the extreme forms"?

Of Dr. Lönnberg's second paper—"Note on the Individual Variation of the Common Hedgehog"—I have less to say. I had found what I thought to be solid points of differentiation between the skulls of hedgehogs from England and Scandinavia, whereby all the examples included in a fair series contained in the British Museum of Natural History were readily distinguishable. Relying on the rule, soon learnt in working at mammals, that such differences, even if slight, are usually not meaningless, I assumed that they would be borne out by a larger series of specimens than I at that time had before me. Dr. Lönnberg finds that this is not so; and I can only say that, while I am sorry that my opinion seems to have been erroneous, I am only too glad to find here in the hedgehog another check to those who, while refusing to recognize colour differences, pin their faith with an inconsistent fidelity on what are frequently the shadowy characters of the cranium. Such characters of the cranium, as every year only seems to teach us, may be full of value or worthless just according to the individual idiosyncrasy of the animal in which they occur. They are in many cases not one bit more reliable than those presented by colour, proportions, or size. In fact, in regard to some of the more important cranial characters, such as those of the

dentition of the vole *, 25 per cent. of specimens examined may be abnormal, a fact which, when still larger numbers are available, may yet prove the saving of my subdivision of the hedgehogs.

In conclusion, I must thank my critic for the exceedingly temperate and forbearing way in which his remarks are couched. Criticisms thus conscientiously formed and fairly expressed cannot surely fail to advance our science.

BIBLIOGRAPHICAL NOTICES.

Zoological Results based on Material from New Britain, New Guinea, Loyalty Islands, and elsewhere, collected during the Years 1895, 1896, and 1897, by Arthur Willey, D.Sc. Lond., Hon. M.A. Cantab. Part IV. Cambridge, May 1900.

THE long-delayed fourth part of Dr. Willey's 'Zoological Results' is now before us and proves fully equal, both in interest and in the general excellence of its contents, to its predecessors. It contains ten memoirs, the majority of which are devoted to reports on the collections made by Dr. Willey in various groups of the animal kingdom. Three, however, are on subjects of morphological interest. The first of these is the opening paper of the volume by Mr. J. Stanley Gardiner, "On the Anatomy of a supposed new Species of *Cænopsammia* from Lifu." Mr. Gardiner divides his subject into four heads, dealing respectively with the general anatomy of the skeleton and specific description, the general anatomy of the polyps, minute anatomy, and some conclusions relating to the body-layers in the Actinozoa. He comes to the conclusion that the whole filament of the primary and secondary, and probably also that of the tertiary, mesenteries is ectodermic in origin, and that the whole of the digestion of the animal is performed by these filaments, and draws the important deduction that the stomodæum of Actinozoa is not comparable with that of the Triploblastica, but is rather, with the mesenterial filaments, the homologue of the whole gut. The so-called endoderm is homologous with the mesoderm of Triploblastica, and the Actinozoan polyp ought to be regarded as a Triploblastic form.

The second of the morphological papers is by Mr. J. J. Lister on *Astrosclera Willeyana*, the type of a new family of sponges. This is a very remarkable organism, with a massive calcareous skeleton of polyhedral elements united to form a rigid skeleton and excluding the soft parts, an arrangement which is only approached among living sponges in the genus *Petrostoma*. Among several points in which *Astrosclera* differs from the rest of the Porifera may be mentioned the absence of a central atrial space, the minute size of the flagellated chambers, and the peculiar form of the flagellated cells,

* As shown by Mr. G. S. Miller, Jun.